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justifiable for realizing terminable annuities : so wayward, from the incidents of taxation, have their relative values become.

I have thus endeavoured to rapidly bring under review some of the leading features of the methods at present adopted for the valuation of Government securities. It is a subject, I think, that will be at once admitted as a proper one for discussion among ourselves, and the more so because it very forcibly illustrates the imperfection of that view of actuarial education which relies on fixed tables of value alone, and fails to inculcate the study of those great public and fiscal questions upon which the practical bearing of all such tables so mainly depends.

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*On the Analogy existing between the aggregate Effects of the Operations of the Human Will and the Results commonly attributed to Chance. By WILLIAM A. GUY, M.B. Cantab.; Professor of Forensic Medicine, King's College; Physician to King's College Hospital; and one of the Honorary Secretaries of the Statistical Society.*

[Read before the Institute of Actuaries, 28th May, 1855, and ordered by the Council to be printed.]

I HAVE not found it easy to select a proper title for this paper ; and I am very conscious of the difficulty of explaining my reasons for instituting the somewhat laborious experiments of which I am now to state the results. Perhaps, indeed, I ought to apologize for offering to the Institute of Actuaries a communication which some of its members, who are conversant with the leading treatises on the doctrine of probabilities, may know to have been anticipated and rendered unnecessary by the labours of men much better qualified than I can pretend to be, to do justice to so profound a subject. I have some reason, however, to believe that the experiments I am about to describe are new ; inasmuch as, though I have consulted one or two works which are likely to have contained some reference to such experiments, had they been already made, and have questioned more than one eminent member of your Society upon the subject, I have not been able to learn that any such experiments are upon record.

In perusing the works of M. Quetelet, it is impossible not to be struck with the remarkable analogies which he proves to exist between events brought about by the operation of the human will

and events due to physical forces over which man can exercise no sort of control ; nor can we easily avoid sympathizing with him in the feeling which dictated the following well known sentiment :—  
 “It must be confessed that, distressing as the truth at first appears, if we submit to a well-followed-out series of observations the physical world and the social system, it would be difficult to decide in respect to which of the two the acting causes produce their effects with most regularity.”\* On reflection, however, this feeling of distress or disappointment, no less than the very natural misgiving which springs up in the mind lest this close resemblance of the effects of moral and physical causes might be turned to the injury of morality and religion, is gradually weakened, and at length disappears. We come at last to perceive and admit that the reproduction year by year of nearly the same figures as the ultimate expressions of the workings of the will in a multitude of persons, need not excite more surprise than the annual recurrence of nearly the same proportionate number of births or deaths in the population of the same country, or even than the maintenance of nearly the same rate of profit year by year in some gambling speculation. The numbers which express the aggregate or ultimate results of the conjoint operation of a number of causes of variable and inappreciable intensity may be expected to present many analogies and coincidences, though the causes be ever so different in their nature, and though the events or actions in which they issue be termed physical in one case and moral in another.

Upon some such considerations as these—upon the assumption that there is a strict analogy between numerical results which are usually attributed to chance, and others which are very commonly treated as beyond her domain—writers on the application of the doctrines and rules of the theory of probabilities to actual practice have acted without hesitation. Thus, Gavarret, an able French author upon medical statistics, criticises with some severity the conclusions of M. Louis respecting pulmonary consumption and fever, on the score of the insufficient number of his facts, and insists on applying to those conclusions corrections avowedly drawn from treatises on the doctrine of probabilities. Now, unless I am greatly mistaken, no attempt of any kind has yet been made to show that rules and calculations derived from abstract reasonings upon probabilities, backed by a few experiments on occurrences brought about by what is commonly designated “chance,” are

\* Sur l’Homme, et le Développement de ses Facultés. Par M. A. Quetelet. Conclusions, book iii. chapter 3.

applicable to events of a totally different order, brought about by the operation of the human will or by the multitudinous external influences which, acting on the human frame, preserve it in health or give rise to the diseases which impair its vigour and ultimately destroy it.

By some such reflections as these I was led to plan and execute the experiments of which I am now to describe the nature and detail the results. It occurred to me that as, when we make use of the word "chance," we really mean, if we mean anything, the result of the combined operation of a number of physical causes of which we are unable to measure the intensity; so we might find a counterpart of such result in the combined operation of mental or moral causes determining the actions of mankind in the aggregate. In order to put the soundness of this opinion to the test, I made the following experimental comparison:—I extracted from the out-patient books of King's College Hospital, in forty successive groups of 25 facts each, the number of men and the number of women in the order in which they were entered on the books. On summing up the result for men and women respectively of the thousand facts thus abstracted, I found that for 369 men attending as out-patients there were 631 women. Assuming this proportion of 369 men to 631 women, as derived from 1,000 facts, to be the true proportion, and looking upon the relative attendances of men and women taken by the 25 to be the ultimate result of the combined operation of a vast variety of physical and moral causes, of inappreciable intensity, acting together—physical causes determining the proportion of illness prevailing among persons of the two sexes; and moral causes such as a sense of convenience in respect of distance, leisure, and opportunity; or preference for the particular Hospital or the particular physician, arising out of former experience, or general repute, or the recommendation of friends—I was curious to know whether, if I substituted for such moral causes as these the equally inappreciable and variable movements of the hands which determine results in games of chance, or the more common experiments in illustration of the doctrine of probabilities, I should obtain similar results. The plan that I adopted with a view to the solution of this question was the following:—I furnished myself with 1,000 peas, of which 369 were white, and represented the number of male patients; and 631 black, and corresponded to the number of female patients. These peas I put into a large bag, which I shook and stirred till its contents were thoroughly mixed, and then withdrew them blindfold by small handfuls at a time, dropping them one by one upon

a surface of paper up to the number of 25, and restoring the surplus or making up the number by fresh drawings as the case might be. After every withdrawal the contents of the bag were stirred and shaken. I made two experiments in this way; the experiments differing in this, that in the first experiment the bag was gradually emptied, its contents being transferred by twenty-fives to another bag, while in the second experiment the drawings were always returned into the bag. The results of these two experiments will be found in the subjoined table, arranged side by side with the corresponding figures as abstracted from the Hospital books. The table is so arranged as to exhibit at a glance all the correspondences between the two experiments and the abstracts.

TABLE I.

| Attendances<br>of Men,<br>in 25<br>Attendances. | White Balls.<br>First<br>Experiment. | White Balls.<br>Second<br>Experiment. | Attendances<br>of Men,<br>in 25<br>Attendances. | White Balls.<br>First<br>Experiment. | White Balls.<br>Second<br>Experiment. |
|---|--------------------------------------|---------------------------------------|---|--------------------------------------|---------------------------------------|
| 0   | —                                    | —                                     | —   | 9                                    | 9                                     |
| —   | —                                    | 3                                     | —   | 9                                    | 9                                     |
| —   | —                                    | 3                                     | —   | 9                                    | 9                                     |
| 4   | 4                                    | 4                                     | —   | —                                    | 9                                     |
| 4   | —                                    | 4                                     | —   | —                                    | 9                                     |
| 4   | —                                    | —                                     | —   | —                                    | 9                                     |
| 5   | 5                                    | 5                                     | —   | —                                    | 9                                     |
| 5   | 5                                    | 5                                     | —   | —                                    | 9                                     |
| 5   | 5                                    | 5                                     | 10  | 10                                   | 10                                    |
| 5   | —                                    | —                                     | 10  | 10                                   | 10                                    |
| 6   | 6                                    | 6                                     | 10  | 10                                   | 10                                    |
| 6   | 6                                    | 6                                     | 10  | 10                                   | —                                     |
| —   | —                                    | 6                                     | 10  | 10                                   | —                                     |
| 7   | 7                                    | 7                                     | —   | 10                                   | —                                     |
| 7   | 7                                    | —                                     | —   | 10                                   | —                                     |
| 7   | 7                                    | —                                     | 11  | 11                                   | 11                                    |
| 7   | 7                                    | —                                     | 11  | 11                                   | 11                                    |
| 7   | 7                                    | —                                     | 11  | —                                    | 11                                    |
| —   | 7                                    | —                                     | 11  | —                                    | —                                     |
| —   | 7                                    | —                                     | 12  | 12                                   | 12                                    |
| —   | 7                                    | —                                     | 12  | —                                    | 12                                    |
| 8   | 8                                    | 8                                     | 12  | —                                    | 12                                    |
| 8   | 8                                    | 8                                     | 13  | —                                    | 13                                    |
| 8   | 8                                    | 8                                     | —   | —                                    | 13                                    |
| 8   | 8                                    | 8                                     | 14  | —                                    | 14                                    |
| 8   | 8                                    | —                                     | 14  | —                                    | 14                                    |
| —   | 8                                    | —                                     | —   | —                                    | —                                     |
| —   | 8                                    | —                                     | 16  | —                                    | 16                                    |
| —   | 8                                    | —                                     | —   | —                                    | 17                                    |
| —   | 8                                    | —                                     | —   | —                                    | 17                                    |
| —   | 8                                    | —                                     | 18  | —                                    | —                                     |
| —   | 8                                    | —                                     | 18  | —                                    | —                                     |
| —   | 8                                    | —                                     | 18  | —                                    | —                                     |
| 9   | 9                                    | 9                                     |   |                                      |                                       |

There is room for some difference of opinion as to the degree of similarity which this table establishes between the results of the operation of the two classes of causes, moral and physical, upon the same number and the same proportion of men and women on the one side, and of white and black balls on the other; but certainly the coincidences are sufficiently numerous to justify the experiments, and to prove the idea in which they originated to be not altogether unreasonable. For if we reckon up the coincidences in the table, we find that the three groups of 40 figures each yield no less than 18 triple coincidences, while the coincidences between the abstract column and the first and second columns respectively are so considerable as 25 in number;\* and this last number of coincidences will appear the more remarkable when I add, that the coincidences between the figures in the second and third columns, which show the results of two modes of drawing, are only 21 in number. Those who are familiar with experiments of this class, and with the variable manner in which figures obtained by precisely the same process from variable numerical elements group themselves, will be ready to admit that the coincidences between two columns of figures obtained by identically the same process might not exceed in number those which Table I. presents.

Having observed, even before I began to arrange the foregoing figures in tables, an amount of coincidence which seemed to justify a further prosecution of this inquiry, I determined to extend it in a direction which might afford some interesting results irrespective of the light thrown on the question under examination. I accordingly selected the number of cases of pulmonary consumption presenting themselves among the out-patients of the Hospital relatively to the number of all other diseases, looking on the consumptive patient and all other patients as a compound class of persons forming part of a community attracted to the Hospital by a variety of motives, just as the men and women of the first experiment were attracted thither. I abstracted the facts from the books in groups of 25, as before; and as the number of cases of consumption, in comparison with cases of all other diseases, proved to be small, I continued my abstracts till I had collected 5,000 cases. Of these 5,000 cases, 172 proved to be cases of pulmonary consumption, and 4,828 cases of other diseases, the proportion being 1 to 28. These cases of consumption I represented by white peas, and the other diseases by black ones; and, after mix-

\* These numbers are exclusive of three instances in which the same numbers (1, 2, and 15) do not occur in either of the columns.

ing them well together, proceeded to draw them out blindfold one by one, arranging them in a row on a groove of paper, that I might note the coincidences of two or more white balls coming together, as I had already noticed the like coincidences in abstracting the cases from the Hospital books. Having some reason to believe, from the large proportion of white balls in some of the last drawings, that the peas had not been sufficiently mixed together, I repeated this experiment; so that in the following table, as in Table I., I am able to compare one abstract with two experiments.

TABLE II.

| Cases of Consumption in 25 Cases. |    |                   | White Balls in 25 Drawings. |                  |              |
|-----------------------------------|----|-------------------|-----------------------------|------------------|--------------|
| Number of Cases.                  |    | Number of Groups. | No. of White Balls.         | No. of Drawings. |              |
| 0                                 | in | 101               | 0                           | Experiment 1     | Experiment 2 |
| 1                                 | "  | 50                | 1                           | 92               | 83           |
| 2                                 | "  | 32                | 2                           | 73               | 70           |
| 3                                 | "  | 12                | 3                           | 18               | 35           |
| 4                                 | "  | 4                 | 4                           | 10               | 10           |
| 5                                 | "  | 0                 | 5                           | 4                | 2            |
| 6                                 | "  | 1                 | 6                           | 2                | 0            |
| 7                                 | "  | 0                 | 7                           | 0                | 0            |
|                                   |    | 200               |                             | 200              | 200          |

In this table, again, there is such an amount of coincidence as would appear to justify the theory which led to the performance of the experiments. The greatest number of white balls in any drawing of the two experiments amounts to 7 in the one experiment and 4 in the other, while the greatest number of consumptive cases in any group of attendances is 6; and though there is not any very striking numerical coincidence, there is at least this degree of resemblance, that large numbers correspond to large numbers and small figures to small figures. Perhaps, too, when it is borne in mind that the cases of consumption are less than 1 in every 25 cases of all diseases, a greater amount of coincidence is not reasonably to be expected; and I regret that I did not substitute groups and drawings of 50 for groups and drawings of 25. This omission may, however, be still repaired, by bracketing together by twos the groups and drawings which follow each other in the records of the abstracts and experiments. The results of thus substituting groups and drawings of 50 for groups and drawings of 25 will be seen in the following table:—

TABLE III.

| Cases of Consumption in each 50 Cases. |                | White Balls in each Drawing of 50. |                  |              |
|--|----------------|------------------------------------|------------------|--------------|
| Number of Cases.                       | No. of Groups. | No. of White Balls.                | No. of Drawings. |              |
| 0                                      | 19             | 0                                  | Experiment 1     | Experiment 2 |
| 1                                      | 31             | 1                                  | 26               | 17           |
| 2                                      | 24             | 2                                  | 31               | 29           |
| 3                                      | 17             | 3                                  | 21               | 26           |
| 4                                      | 5              | 4                                  | 9                | 18           |
| 5                                      | 2              | 5                                  | 6                | 8            |
| 6                                      | 2              | 6                                  | 3                | 2            |
| —                                      | —              | 10                                 | 2                | 0            |
|  |                |                                    | 2                | 0            |
|  | 100            |                                    | 100              | 100          |

When the two orders of facts—the cases from the Hospital books, and the drawings of balls from the bag—are treated in this manner, it is impossible to overlook the numerous coincidences which exist between them. If we take the second experiment, as being, for reasons already stated, the most trustworthy, the coincidences are extremely striking; for though the relative number of cases of consumption is not exactly the same as the relative number of white balls, the differences are very slight. For 19 we have 17; for 31, 29; for 24, 26; for 17, 18; and for two groups of 5, two drawings of the same number. Even the first and least complete experiment exhibits two complete coincidences (31 against 31, and 2 against 2), and one or two close approximations.

I have already stated that, in performing these experiments on the relative number of white and black balls, representing the relative number of cases of consumption and of all other diseases, I took care to arrange the balls in such a manner that I might note the coincidences and compare them with those which were found to take place in the abstracts from the Hospital books. The results obtained in this manner are shown in the following table:—

TABLE IV.

|   | Cases of Consumption. | White Balls.  |               |
|---|-----------------------|---------------|---------------|
|   |                       | Experiment 1. | Experiment 2. |
| Sequence of two cases or }<br>two balls . . . . . }     | 13                    | 4             | 3             |
| Sequence of three cases }<br>or three balls . . . . . } | 2                     | 1             | 0             |

I must confess that I had expected to find a greater similarity in the figures of the three columns of this table. I was prepared



to find as many, or nearly as many, drawings of 2 and 3 white balls in succession, as I had found of cases of consumption entered consecutively in the books. The difference in this case may perhaps have arisen from a circumstance which may to a certain extent have affected both my abstracts from the Hospital books. Men suffering from the symptoms of consumption may have been brought together in the Hospital waiting room, and have followed each other into the physician's room, just as men and women are constantly, for convenience sake, sent into the physician in small groups of either sex. Be this as it may, the results are wider apart than I had expected to find them.

There is still one other comparison which it occurs to me to make, as having some bearing upon the question which I am now examining. The comparison in question is between the number of consecutive groups of 25 facts in which 0, 1, or 2 consumptive cases, or 0, 1, or 2 white balls, presented themselves, in the one abstract and the two experiments. This comparison is made in Table V.

TABLE V.

| Cases of Consumption,<br>or White Balls. |    |   | Number of Times.        |                      |                       |
|--|----|---|-------------------------|----------------------|-----------------------|
|  |    |   | Hospital<br>Attendance. | First<br>Experiment. | Second<br>Experiment. |
| 0  | in | 2 | 15                      | 12                   | 15                    |
| 0  | "  | 3 | 6                       | 8                    | 2                     |
| 0  | "  | 4 | 2                       | 4                    | 1                     |
| 0  | "  | 5 | 1                       | 1                    | 0                     |
| 0  | "  | 6 | 1                       | 0                    | 1                     |
| 0  | "  | 7 | 0                       | 1                    | 1                     |
| Total..                                  |    |   | 25                      | 26                   | 20                    |
| 1  | in | 2 | 7                       | 14                   | 10                    |
| 1  | "  | 3 | 0                       | 3                    | 5                     |
| 1  | "  | 4 | 0                       | 0                    | 0                     |
| 1  | "  | 5 | 1                       | 0                    | 0                     |
| 1  | "  | 6 | 0                       | 1                    | 0                     |
| 1  | "  | 7 | 0                       | 1                    | 0                     |
| Total..                                  |    |   | 8                       | 19                   | 15                    |
| 2  | in | 2 | 5                       | 0                    | 2                     |
| 2  | "  | 3 | 0                       | 0                    | 0                     |
| 2  | "  | 4 | 0                       | 1                    | 0                     |
| Total..                                  |    |   | 5                       | 1                    | 2                     |

Here, too, the resemblance between the abstracts and the two experiments is not very considerable, unless it be in the figures

that represent the number of consecutive groups of 25 cases or 25 balls in which there was no case of consumption or no white ball. If we limit our attention to the abstracts of cases and the second and more complete experiment, we find that there are 15 instances in either column of results in which there was no case of consumption and no white ball twice consecutively, and also one instance in which there was no case of consumption and no white ball six times consecutively. In the other four horizontal columns of figures there is more or less divergence. In the other divisions of the table, which show the numbers of instances in which 1 or 2 cases of consumption, or 1 or 2 white balls, presented themselves in consecutive groups of 25, the divergence of numbers is considerable. Still, when we take into account the considerable differences which would most certainly have shown themselves between three sets of abstracts of attendance at hospital, and that do show themselves between two experiments differing only in the degree to which the blending of white and black balls was effected, there is certainly nothing in this table to militate against the theory which led to the making of these experiments, but, on the contrary, a fair confirmation of the soundness of the views which dictated them.

On a careful consideration, then, of the experiments which I have brought forward in this essay, and bearing in mind the great differences which are found to prevail between the aggregate results of successive observations and experiments on precisely the same order of facts, and made in exactly the same manner, I think that I am justified in asserting that there is a very marked analogy existing between the aggregate effects of the operations of the human will and the results ordinarily attributed to chance. In other words, there is reason to believe that a very marked analogy exists between the results brought about by the volition of a considerable body of individuals set in motion by moral motives, and the results brought about by those physical movements of one, two, or more individuals which bring about the results of games of chance, or of experiments made in illustration of the doctrine of probabilities.

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